by some high authorities, including himself, Mr. Darwin, and Mr. James Geikie. While we may all share in this congratulation, it must be remembered what it involves.

It has been the fashion with an extreme and aggressive school of glacialists to postulate an excavating tendency in ice to which the formation of lake basins and valleys-without-outlets in mountain districts has been attributed. They will not allow that rock basins are due to any other cause than "omnipotent ice." They scoff at explorers of the mechanics of ice in Alpine countries, like Prof. Bonney and Mr. E. Hill. They jeer at those who have devoted much patience to unravelling the mysteries of Plutonic action, like Prof. Judd and others, who attribute a large number of lakes to dislocations and to foldings of the subjacent rocks. It is no use, in arguing with them, to refer to mechanical difficulties like those involved in conveying thrust of more than a certain amount through a substance like ice, which is known to crush under a moderate pressure, nor to produce any number of mechanical arguments against the capacity of ice to erode lake basins such as those in question; nor is it any use appealing to the stupendous geological difficulties against their conclusions which have been accumulated by quite a number of skilled geologists at home and abroad. All these efforts are futile, for we are told that the ice to which appeals must be made is quite a different thing to any ice we can experiment upon or examine, and that it must not therefore be measured by the ordinary laws that govern ice such as we know it, and this appeal to transcendental ice is considered to be orthodox science in the nineteenth century, an age when induction is supposed to have become a supreme law to us all, and when *à priori* postulates are generally discarded from the realm of physical research. Let this pass, however, and let us test the question in another way. Let us test it, in fact, by this very case of Brazil.

There has never been a glacial period in, nor are there traces of glacial action in the highlands of, Brazil, we are told by Dr. Wallace. Granted. How then can Dr. Wallace, and those who agree with him in this matter, explain the existence on the plateau of Bahia of perhaps the largest and most remarkable collection of rock basins in the world, rock basins existing, too, in close juxtaposition with most perfect examples of giants' cauldrons on the largest scale. This is assuredly a dilemma for the transcendental school of geologists.

Let me quote from Mr. Allen's graphic descriptions of these rock basins. Speaking of the plateau of Bahia, he says: "Over this whole region there is an almost entire absence of loose materials on the surface . . . slight knolls and shallow basins alternate which rarely differ more than 20 or 30 feet in elevation. In the rainy season many of these basins become filled with water, forming shallow lagoas varying in area from less than one to more than 50 acres, from most of which the water evaporates in the dry season . . . . So numerous were these lagoas for more than 50 miles that it seemed natural to speak of this region in my notes as the "Lake Plain." Almost everywhere the elevations are evenly rounded, indicating that the rocky crust has been exposed to rain and probably long continued abrasion. But the absence of abraded materials seemed most remarkable; very rarely were even loose boulders observed, though a few such were repeatedly noticed. At frequent intervals there were irregular holes in the rocks, usually nearly filled with water, to which the inhabitants give the name of 'caldeiraos.' These caldeiraos are of frequent occurrence. . . . . Nearly all of the considerable number examined proved to be genuine pot-holes, and some of them were of great size. The largest one I measured was elliptical in outline, 18 feet long, 9 or 10 in width, and 27 deep, with smoothly worn sides. . . . These pot-holes often occur out on the plain, far away from any high land, and they are sometimes found excavated on the summits of slight bulgings in the plain, or even on the top of a hill."

I would ask, in all seriousness, whether, if phenomena like these had been described from the Alps or from Nova Scotia, they would not assuredly have been pointed to by extreme glacialists as the unerring footprints of great ice-sheets, and yet Dr. Wallace, who is a champion of the school, repudiates the former glaciation of Brazil altogether.

What is to be said in regard to this dilemma then? It is quite clear that either the facts must be disputed (and who is to dispute them?), or else the champions of ice at-all-hazards must concede that rock basins and giants' cauldrons can be made by other agencies than ice. If so, they can be made as well in one place as in another. If they could be made by other causes on the plateau of Bahia, why not in the highlands of Tasmania?

I am bound to say I was taken aback by Dr. Wallace's comments on a letter from one of your correspondents, which appeared in NATURE a short time ago. That gentleman professed to make an exploration of certain parts of Tasmania with another experienced geologist. They were both champions of the glacial theory. They both went prepared to find traces of glacial action there, and certainly in our latitudes no evidence seems more easily discriminated, and they came back convinced that in the districts where the rock basins of Tasmania abound, there are no traces of glacial action to be seen. They could find none. Mr. Johnstone, who has written an elaborate and detailed geological memoir on the island, and who has explored it in many directions, could find none either, save on the western flanks and in the valleys of the Tasmanian Alps in the western part of the island, where it has been long known that traces of former local glaciers exist. There is absolute unanimity among the native geologists that nothing in the shape of ice-sheets existed there, and there is no ice-spoor in the central districts where the great Tasmanian lakes occur. Dr. Wallace's answer to all this was certainly unexpected. He has not himself visited the island, and yet he disputed not only the inferences but the facts and the observations. Why should the voice of Esau be listened to and approved in Brazil, and that of Jacob be repudiated in Tasmania? Mr. Johnstone and the other observers in Tasmania are assuredly to be trusted in such an issue quite as much as Prof. Branner. I cannot see on what ground the discrimination is made, except the desperate inconvenience of postulating a glacial nightmare in the tropics.

Assuredly the whole difficulty lies in championing a theory of the origin of lakes, unknown in geology until introduced by Ramsay, whose extravagance at times may be measured by some of his phrases addressed to the British Association when he presided over the geological section. From all sides there comes a revolt against this theory, which is based on no empirical evidence, and is at issue with the mechanical properties of ice so far as we know them, and with the observations of practised observers of the first rank. I am bound to say that those geologists who habitually make appeals to forces in Nature, and to properties of matter which are purely hypothetical and unwarranted by experience, are leading us back to times when Aristotle and deductive reasoning dominated European thought, and when Bacon had not yet taught us better things.

My attention has been called to an oversight in my previous letter. Among those who many years ago did good work in dissipating the particular glacial monster that was generated in the valley of the Amazons, was my old friend Dr. Woodward, whose papers on the subject in the volume of the *Ann. and Mag. of Nat. Hist.* for 1871, pp. 59 and 101, I had overlooked.

HENRY H. HOWORTH.

30 Collingham Place, Cromwell Road, October 27.

Correlation of Solar and Magnetic Phenomena.

I WAS glad to see (NATURE, vol. xlix. p. 2) in the notice of Miss Clerke's "Popular History of Astronomy," that attention was drawn (1) to the correspondence in time between a certain luminous outburst seen on the sun on September 1, 1859, by Carrington and Hodgson, and a disturbance of the magnets at the Kew Observatory; and (2) to the statement of the late Mr. Whipple that the magnetic movement was really a small one, and that in his opinion the observed correspondence was a mere accidental coincidence. Those who have read Carrington's original account (Monthly Notices of the Royal Astronomical Society, vol. xx. p. 13) will remember that at the time he himself did not lean towards hastily connecting the phenomena, remarking that "one swallow does not make a summer." But authors of text-books on astronomy, who may be only to a partial extent observers, are too apt to state the matter in such a way as to give an impression that we have here an undoubted instance of direct connection, instead of a case of apparent connection, to be taken merely for what it is worth, seeing that the occurrence has remained to the present time without corroboration. I should like to take the opportunity to support, in the fullest manner, the opinion of Mr. Whipple, which acquaintance with the Greenwich magnetic registers tells me to be a true one. The magnetic movement in question, as recorded at Greenwich, was similarly small.

But the erroneous impression lives long. May I therefore be

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further allowed to give some reasons for the opinion expressed. That there exists a relation between sun-spots and magnetism is undoubted. And although those who are able to study the variations of sun-spots side by side with the variations of magnetism can very well see to what extent the relation definitely holds, it is difficult adequately to convey to others a due impression of all the circumstances of the case. Periods of maximum sun-spots are periods of great magnetic activity and energy, whilst periods of minimum sun-spots are periods of magnetic quiet. But it has not yet been found possible to trace direct correspondence in details. Thus, when a large spot is present there may occur one or more considerable magnetic disturbances or storms, some enduring it may be for a few hours only, others it may be for several days, but, assuming direct solar influence, what it is that precisely determines when such disturbances shall arise is unknown. Further, at times of sun-spots being numerous, there is also considerable general magnetic irregularity. Now, in these magnetic disturbances and irregularities there will be innumerable individual motions far exceeding in magnitude that accompanying the Carrington sun outburst, and yet during all the many years that have elapsed since 1859, through which period the solar surface has been continuously scrutinised by hundreds of observers in different lands, no second occurrence similar to that of 1859 has come to light. But if there be so close a connection between solar and magnetic phenomena as the occurrence in question would seem to indicate, the fact that we have no corroboration of the solitary observation of 1859 is surely remarkable, considering that, of late years, it is very much to correspondence in details that attention has been to a great extent directed. If irregular magnetic movements were comparatively few, the observation of 1859 might possess some significance, but they are, on the contrary, multitudinous, many at times occurring during the course of a single day, and often of considerable magnitude, but yet without any recorded accompanying solar manifestation.

To sum up, the points of the matter may be thus stated :—

(1) The solar outburst in 1859 was seen independently by two observers : the fact of its occurrence seems therefore undoubted.

(2) The corresponding magnetic movement was small.

(3) Many greater magnetic movements have since occurred.

(4) No corresponding solar manifestation has been again seen, although the sun has since been so closely watched.

The solar outburst of 1859 would thus appear to have been a rare phenomenon, and its observed occurrence at the time of a recorded magnetic movement quite an accidental coincidence.

This conclusion in no way invalidates the question of general relation between sun-spots and magnetism, whatever may be the true explanation of that relation.

Greenwich, November 6. WILLIAM ELLIS.

The Recent Earthquake.

AFTER the Pembroke earthquakes of August 1892, you were good enough to insert a letter from me (vol. xlvi. p. 401) asking for observations from different places. In reply to this letter, I received so many and such valuable records, that I should be greatly obliged if you would allow me to make a similar request for accounts of the recent earthquake of November 2, in Wales and the West of England. I should be very grateful for descriptions from any place whatever. The questions printed below indicate the points on which information is chiefly desired, but if any observers are able and willing to give further details, I shall be pleased to send them my fuller list of questions, which I may remark are somewhat different from those given in the letter referred to above.

(1) Name of the place where the earthquake was observed.

(2) Time at which it was felt, if possible to the nearest minute.

(3) Nature of the shock. (*a*) Were two or more distinct shocks felt, separated by an interval of a few seconds? (*b*) If so, which was the stronger? (*c*) What was the duration (in seconds) of each, and of the interval between them? (*d*) During this interval was any tremulous motion felt or rumbling sound heard?

(4) Duration in seconds of the whole shock, not including the accompanying sound.

(5) Was the shock strong enough (*a*) to make doors, windows, fire-irons, &c., rattle ; (*b*) to cause the chair, &c., on which the observer was resting to be perceptibly raised or moved ; (*c*) to make chandeliers, pictures, &c. swing, or to stop clocks?

(6) (*a*) Was the shock accompanied by any unusual rumbling sound, and, if so, what did it resemble? (*b*) Did the beginning of the sound precede, coincide with, or follow the beginning of the shock, and by how many seconds? (*c*) Did the end of the sound precede, coincide with, or follow the end of the shock, and by how many seconds? (*d*) Were the strongest vibrations felt before, at, or after the instant when the sound was loudest, and by how many seconds? CHARLES DAVISON.

373 Gillott Road, Birmingham, November 6.

"An Ornithological Retrospect."

I HAVE been interested in reading "An Ornithological Retrospect," by your correspondent, Dr. Sharpe. His reference to myself by name in the concluding paragraph is partly my excuse for troubling you with a few remarks upon this article. Dr. Sharpe, in one long breath, deplores (pleonastically) the fact that "very little anatomical work has scarcely been done" recently in ornithology, and exults over a reviewer in a "leading London paper," who apparently took the same view —tomahawking him with the remark that "in every branch of the subject considerable progress has been made." I think that the opinion of the minority in this case is correct, and that our knowledge of bird anatomy is progressing. But those of us who are occupied with this study have frequently to regret the ignoring of anatomical facts by systematists ; this is particularly discouraging, since by far the larger proportion of papers upon bird anatomy are purely of systematic interest, dealing with the resemblances between bird and bird. Dr. Sharpe evidently feels that the British Museum Catalogues of Birds are not beyond criticism from this point of view. In one or two volumes there is a conspicuous absence of any arrangement in accordance with anatomical fact. Dr. Sharpe, therefore, is rather imprudently candid in saying that to understand these catalogues a man must be an ornithologist.

Zoological Society's Gardens. FRANK E. BEDDARD.

The Foam Theory of Protoplasm.

IN your issue of October 19 there appeared, under the title "Bütschli's Artificial Amœbæ," a review, by Dr. John Berry Haycraft, of Prof. Bütschli's work upon protoplasm. I venture to think that in many places Dr. Haycraft has misrepresented entirely Prof. Bütschli's researches, while other objections or criticisms which he brings forward are answered in the book itself. Since I have been engaged for some time upon a translation of Prof. Bütschli's work, which is now in the press, I must ask your readers to suspend their judgment until they have a better opportunity of forming an opinion for themselves.

2 Blackhall Road, Oxford. E. A. MINCHIN.

SCIENCE IN THE MAGAZINES.

AMONG the magazines received by us, the *Fortnightly* is well to the front as regards articles having a scientific interest. Dr. Alfred R. Wallace writes on "The Ice Age and its Work," with the object of explaining "the nature and amount of the converging evidence demonstrating the existence of enormous ice-sheets in the northern hemisphere, to serve as a basis for the discussion of the glacial origin of lake-basins, which will form the subject of another article." After briefly describing the foundation of the science dealing with glaciers and their action, and the early school of glacialists, Dr. Wallace states the phenomena which points to the former existence of glaciers in regions where the mountain-tops are at present below the snow-line. These are classified as follows :—(1) Moraines and drifts ; (2) Rounded, smoothed or planed rocks ; (3) Striæ, grooves, and furrows on rock-surfaces ; (4) Erratic and perched blocks. As a good example of a moraine, that in Cwm Glas, on the north side of Snowdon, is mentioned, together with those in Glen Isla (Forfarshire), and the Troutbeck Valley near Windermere. In Cwm Glas, also, smoothed and rounded rocks are to be seen above the moraine. Striated, grooved, and fluted rocks are exemplified by those near the lakes of Llanberis, and by the remarkable effects exhibited at Kelly's Island, at the western end of Lake Erie. The enormous block near St. Petersburg, and the mass of Swedish red granite found at Fürstenwalde, south-east of Berlin, are given as in-

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